Comments

In general, it looks like my specific but minor comments are carefully taken into account. However, I am still concerned about the issues listed below and these should be satisfactorily addressed before the paper can be accepted for the publication.

First of all, contents are largely expanded particular in Sections 2 and 3 according to the referee's suggestions including myself. Contents newly added on the manuscript are probably based on the logbook recorded in Antarctica. In actual they are quite useful to recognize the situation during the observation periods. However, the descriptions are now too long and rather redundant. Since this is not a data-report but a scientific paper, authors need to set the focus on the specific issues. Redundant part should be eliminated and contents should be made much more straightforward. In fact, conclusions are deduced from the data obtained on group F only and others are not included in the analysis. Thus, detailed introductions about group A to E are not always necessary.

Further, if the authors would like to emphasize the points that drifting snow is necessary for wind-packing (but is not always sufficient) and subsequent drifting snow event increases in surface hardness, Figs. 6 and 7 will be fully enough to come to the conclusions. I do not think the following analysis and the devious explanations are needed. Although I can appreciate the efforts very much, unfortunately they did not work as were expected and all outcomes seem weak to persuade the readers. I can recognize quite well that the observations in the field, in particular under the harsh conditions in Antarctica, measurement conditions and possible observation period were extremely restricted. Such excuses are also found in the manuscript. Probably due to these limitations, the discussion parts are not straightforward and no distinct evidences are found out.

It will be surely useful when the manuscript discusses the snow dune formations; the distributions of snow hardness, topography from tail to crest are finely observed probably for the first time. However, when the authors would like to investigate the wind-packing as shown in the title, the contents after the chapter 3.4 should be shortened largely.

Anyway, at this stage, authors still have a long way to get close to the final sentence in Abstract, "These results form an important step in understanding how drifting snow links precipitation to deposition via snow hardening.".

Specific comments are listed below.

Figure 4: Date shown on the bottom of the figure should be clarified as we can recognize the date specifically, as is shown in Figure 6.

Page 9, line22: “Even after the main drifting snow event, there are many SMPs with very soft snow at the surface. This shows that drifting in itself is not a sufficient condition to form a wind crust”. Would you like to say that the drifting snow is necessary condition but is not always sufficient condition? If it is the case, I strongly recommend to declaring like this, such as in the abstract and conclusion.

Page 11, line 4: “therefore not further analyzed” Similar notes can be found repeatedly in the manuscript. When the accuracy of the data is not enough and you do not use the data in the analysis, I am not certain it should be fully declared. As is mentioned before, setting the focus on the measurements at group F looks better strategy.

Page 18, line 5: “The observed period” What do you mean by that? Duration of the observation period in the wind tunnel and in the Antarctica is the same? Perhaps it is not true.

Page 19, line 8: “This is most likely due to higher wind speeds and more intense drifting in Antarctica. This leads to more frequent and more powerful impacts of snow particles on the surface causing more compaction and hardening” This is probably true, but there seems a gap in the argument. Preferably this speculation needs sort of proof quantitatively or even qualitatively.

Page 19, line 26: ‘Time and sintering are not the dominating processes in wind-packing and is due to the impact of snow particles”. I am just curious whether the authors have tried X-rays CT analysis for the surface snow in the wind tunnel experiment. I believe it will give us the useful information to investigate the dominant process.

Line 15: “We are not suggesting that wind exposure is not an important factor for wind-packing in Antarctica”. Do you mean the effect of wind cannot be excluded? It actually appeared abruptly and I could not follow what leads this declaration. Is this inconsistent with the wind tunnel experiments in which the wind-packing was not formed without drifting?